**From:** em.agee.0.82cf3f.49202e0f@editorialmanager.com <em.agee.0.82cf3f.49202e0f@editorialmanager.com> on behalf of Agriculture, Ecosystems & Environment'' <em@editorialmanager.com>  
**Sent:** Friday, April 21, 2023 4:25 AM  
**To:** 何傳愷  
**Subject:** Decline: with option to resubmit: AGEE36058

Ms. Ref. No.: AGEE36058  
Title: A predator in need is a predator indeed: generalist arthropod predators function as pest specialists at the late growth stage of rice  
Agriculture, Ecosystems and Environment  
  
Dear Professor HO,  
  
Thank you for your interest and submission to Agriculture, Ecosystems and Environment. Unfortunately, based on evaluation, this manuscript, in the present state, is not suitable for publication in Agriculture, Ecosystems and Environment. The reasons for this decision are explained below in the Editor's comments.  
  
However, resubmission of a thoroughly revised manuscript may be considered. In the revision, all issues raised in the review process should be addressed adequately. If you choose to submit a revision, please include the manuscript number of this current submission and a point by point response to the comments by the editor and/or reviewers in the resubmission cover letter.   
The resubmission will be treated as a new manuscript and go through the complete review process.  
Thank you for choosing our journal as a possible publication medium.  
  
Kind regards,  
Audrey Alignier, PhD  
Editor  
Agriculture, Ecosystems and Environment

Editor's comments:

Thanks for your submission of the manuscrcipt AGEE36058 titled ‘A predator in need is a predator indeed: generalist arthropod predators function as pest specialists at the late growth stage of rice’. This manuscript has been reviewed by two independent experts and their comments are copied below. As you will see, reviewers find the topic interesting however they find too much to criticise in relation to your manuscript which compromises its suitability for publication in AGEE. In particular, objectives and hypotheses are unclear. You should insist on the novelty of your study. The unbalanced design is questionable. More importantly, the method you used appears inappropriate : pest consumers were pooled at a taxonomic level rather too coarse (order) adequate isotopic signature resolution. The absence of intraguild predation is also highly intriguing and questions the significance of the results (especially how efficient spiders are in pest suppression). About the format, I agree with Reviewer#1 that Introduction and Discussion are excessively long.  For all these reasons, I've resolved to decline the work with the option to resubmit. Should you ultimately choose to resubmit to AGEE, the expectation is that the commentary would be carefully and thoroughly accounted for in the revised manuscript.

With kind regards,

Dr. Audrey Alignier

Reviewer's Responses to Questions

Note: In order to effectively convey your recommendations for improvement to the author(s), and help editors make well-informed and efficient decisions, we ask you to answer the following specific questions about the manuscript and provide additional suggestions where appropriate.  
  
1. Are the objectives and the rationale of the study clearly stated?  
  
Please provide suggestions to the author(s) on how to improve the clarity of the objectives and rationale of the study. Please number each suggestion so that author(s) can more easily respond.

Reviewer #1: Yes

Reviewer #2: 1. Yes, the objectives stated but are entangles with the overall study goal; these need to be disentangled  
2. The objectives imply work on generalist arthropods whereas only 2 groups of these (spiders and ladybeetles) were examined; mots (most) of the other generalist predators in rice-field systems (ants, ground beetles, earwigs, crickets, predatory bugs) were not part of the study CK: 1) We did not cherry pick. These two groups dominated (the sampling results). 2) Still included 7 families, various predators.   
3. No hypothesis is stated, and while this might not be absolutely necessary, it would be useful to add a small bit about what the study's expectations generally were, for the conclusions to be viewed by the general readership from the perspective of whether or not such expectations were met/achieved. Add expectation after our objectives.

2. If applicable, is the application/theory/method/study reported in sufficient detail to allow for its replicability and/or reproducibility?  
  
Please provide suggestions to the author(s) on how to improve the replicability/reproducibility of their study. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Mark as appropriate with an X:  
Yes [X] No [] N/A []  
Provide further comments here:

Reviewer #2: Mark as appropriate with an X:  
Yes [] No [X] N/A []  
Provide further comments here:  
  
1. The sample sizes (number of farms) were unbalanced (3, 7 and 7 in the 1st, 2nd and 3rd years, respectively). There is need to clarify how the impact of this was addressed so as not to invalidate analyses methods applied CK: 1) Not a big issue for current statistical methods. 2) That being said, we did … and address this in Potential Caveats and Appendix.  
2. The sweep-netting strategy needs clarification (whether only ridge-side plants were swept, as the authors state, or a more representative method covering adequate sections of rice-field-plots were sampled). They speak of sweeping while walking along ridges  
3. Need to add if field choice was randomized, and if so, how this was achieved for sampling independence 1) Systematically across landscape (at least x km apart) + Paired organic and conventional, 2) Refer to our Ecosphere paper.  
4. Sampling frequency not stated Based on rice stage, Roughly 3 weeks (?) apart   
5. 1-km buffer around farms tp characterize forest cover effects on predation, is rather too far to have any impact on arthropod assemblages on the farms. the role of forest cover could well have been eliminated from the study 1) Reference to support our analysis, 2) Ask William for comments

3. If applicable, are statistical analyses, controls, sampling mechanism, and statistical reporting (e.g., P-values, CIs, effect sizes) appropriate and well described?  
  
Please clearly indicate if the manuscript requires additional peer review by a statistician. Kindly provide suggestions to the author(s) on how to improve the statistical analyses, controls, sampling mechanism, or statistical reporting. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Mark as appropriate with an X:  
Yes [X] No [] N/A []  
Provide further comments here:

Reviewer #2: Mark as appropriate with an X:  
Yes [] No [X] N/A []  
Provide further comments here:  
1. Pest consumers were pooled at a taxonomic level rather too coarse (order) adequate isotopic signature resolution. Family level would have been much more acceptable Identified to family/genus level, but pooled because of 1) similar isotope signature, and 2) providing enough replicates for better model estimation.   
2. Mention is made of beta regression (GLM or GLMM) but not mention of probability distribution of link function employed in such modelling  
3. Results of Isotopic analyses of food proportions in predators diets using MixSIAR should be presented in form of median with credible intervals rather than means and standard errors; We actually used median for each farm, and average these medians across all farms and rice stages. Confidence interval?

4. Could the manuscript benefit from additional tables or figures, or from improving or removing (some of the) existing ones?  
  
Please provide specific suggestions for improvements, removals, or additions of figures or tables. Please number each suggestion so that author(s) can more easily respond.

Reviewer #1: No

Reviewer #2: 1. There should be at least one table summarizing median and credible intervals of pest proportions in predator diets 1) Our results were generated originally from median. 2) Pleas see Table S2.  
2. Figures from the MixSIAR results should presented in form of posterior density plots of medians, not linear graphs of means 1) Average across all distribution. 2) Provide a few examples in this reply letter. Then explain that we have xx figures (each for a treatment combination). Therefore, it may not be practical to provide all these figures.   
3. Therefore tables and figures presenting results in form of means and standard errors should be removed

5. If applicable, are the interpretation of results and study conclusions supported by the data?  
  
Please provide suggestions (if needed) to the author(s) on how to improve, tone down, or expand the study interpretations/conclusions. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Mark as appropriate with an X:  
Yes [] No [X] N/A []  
Provide further comments here:

Reviewer #2: Mark as appropriate with an X:  
Yes [X] No [] N/A []  
Provide further comments here:  
1. The results as currently presented are based on means and standard errors of MixSIAR models. These carry a high level of inaccuracy because MixSIAR model estimates of food source proportions are typically skewed, making means very vulnerable to effects of statistical tails. Medians are the most resilient to such impacts, but but include credible intervals  
2. The authors state that generalist predators qualify as specialists because they have high proportions of pest food in their diets towards crop maturity, without providing justifiable evidence 1) We provide the percentage, tone down? 2) See if we can find a reference with percentage for more specialized predators (e.g., studies of gut content molecular analysis?)   
3. Throughout the article, there is need to emphasize that the study was based on two groups of generalist predators only - (spiders nd ladybeetles), and not make an overarching generalization for all generalist predators

6. Have the authors clearly emphasized the strengths of their study/theory/methods/argument?  
  
Please provide suggestions to the author(s) on how to better emphasize the strengths of their study. Please number each suggestion so that the author(s) can more easily respond.

Reviewer #1: Yes

Reviewer #2: In general yes

7. Have the authors clearly stated the limitations of their study/theory/methods/argument?  
  
Please list the limitations that the author(s) need to add or emphasize. Please number each limitation so that author(s) can more easily respond.

Reviewer #1: No

Reviewer #2: In the limitations/caveats subsection, the authors state that they are reluctant to imply actual pest suppression because they did not complement isotopic analyses (SIA) with direct field observation. The SIA technique is meant to more or less eliminate the need for direct field observation, or at least render it subliminal. Here its better to say that the study did not incorporate assessment of crop damage rates or yield gap data to support implications of pest suppression

8. Does the manuscript structure, flow or writing need improving (e.g., the addition of subheadings, shortening of text, reorganization of sections, or moving details from one section to another)?  
  
Please provide suggestions to the author(s) on how to improve the manuscript structure and flow. Please number each suggestion so that author(s) can more easily respond.

Reviewer #1: Yes

Reviewer #2: Generally no, except for the need to clearly dis-engage or distinguish the study's mail aim/goal from objectives and hypothesis, putting all these in a stand-alone paragraph under Introduction section

9. Could the manuscript benefit from language editing?

Reviewer #1: No

Reviewer #2: No

Reviewer #1: This field is optioIn this paper the authors investigated the role of generalist and specialist predators in the rice ecosystem. They employed stable isotope approach to test three hypotheses. They found predation on pests increased over season, was similar across years, and was higher in conventional than organic farming. They conclude that generalist predators have potential to produce stable top-down effect.  
  
The study investigates an important topic, the design was adequate and the results are sound but not in current version.  
  
I have two major concerns. At first, the role of generalist spider predators has been known for a long time. So in this respect the study does not bring anything novel. But the quantification of rice pest suppression is new. Yet, this is the main problem of the study. The isotopic analysis is not the best to be used to quantify trophic interactions as it lacks the precision. In particular, the absence of intraguild predation is intriguing. The estimates of predation on pests are thus biased to an unknown degree. This is a serious limit of the study. Unfortunately, the authors acknowledge this limit only in the very last part of Discussion, so the reader is impressed by reading the paper how efficient spiders are in pest suppression. The authors should either provide evidence that intraguild prey was scarce and therefore unlikely to form a significant proportion of the diet or state this limit in the beginning of the study. 1) Address the significance of our study (quantification data to fill knowledge gap), 2) Mention the limitation of stable isotope analysis (intraguild predation) in Introduction. 3) We agree with the review that stable isotope analysis is not as precise as some methods (molecular analysis) for a snapshot, but more likely reflect accumulated results. Not try to champion this method; rather, provide useful data to help the advancement of this study field. 4) Many papers recently have take the advantage of stable isotope analysis, for example, …   
  
In addition I find both Introduction and Discussion excessively long. For example, the first paragraph of Introduction could be omitted as it is too general. We can shorten but not remove it, which provides history background, useful for our readers.   
  
Specific comments:  
Line 18: Rather than biocontrol use Conservation control.  
Lines 86-90: To test the hypothesis of consistency is trivial unless there is a reason why generalists as opportunists would switch to a different prey some years.  
Lines 125-128: Repetition of the former text. Omit it.  
Line 145: I am surprised to read that the conventional farms used only one application of insecticide per season. Is it really true?  
Lines 143, 145: Add information when the insecticides were applied.  
Line 147: How often sweeping was done? Once per growing stage?  
Line 150: To what taxonomic level identification was performed? And how was it identified?  
Line 159: I wonder why there were only 352 predators but 828 prey samples if the study plots were dominated by a single pest species?  
Lines 167-170: Omit definition of guilds.  
Line 189: If samples at seedling stage were later omitted why do you earlier say that you collected data at four growing stages?  
Line 203: How did you estimate the proportion of herbivores? This must have been done by the model. Explain.  
Lines 264-267: This belongs to Discussion.  
Lines 269-270: I am surprised to read the absence of association between proportion of herbivores in the predator body and their abundance in the field. If spiders are generalists then there should be such association. This makes the obtained results suspicious.  
Lines 2741-294: This is just repetition of results. Omit it.  
Lines 331-344: This is trivial. Omit it.  
Lines 357-367: This is trivial. Omit it.nal. If you have any additional suggestions beyond those relevant to the questions above, please number and list them here.  
  
  
Reviewer #2: Article Ref: AGEE36058A predator in need is a predator indeed: generalist arthropod predators function as pest specialists at the late growth stage of rice,  
The study examined rates of consumption of a range of arthropod herbivores (pests) in rice-fields of Taiwan by spiders and lady beetles, by using stable isotopes mixing models 9MixSIAR) to estimate proportional contributions of these pests to predators' diels. They additionally evaluated the influence of some habitat variables in contributing to such pest consumption trends, comparing organic versus conventional farming systems over three consecutive years.  
This is a very significant study, with potential to contribute immensely to the still narrow range of literature on the general subject of new techniques and strategies for quantifying pest biocontrol in cereal crops. It is also very timely, given the increasing value attachment to to non-chemical measures for mitigating crop damage by arthropod pests. The element of crop growth stage as a factor in determining pest consumption levels is especially interesting as it is rarely examined yet arthropod assemblage patterns, and presumably their trophic interactions do change with time  
While the study is generally well designed, it has a number of methodological gaps that would need to be addressed so as to put the results into proper perspective for conclusions to be justifiably drawn. These include wide generalizations about the range of predators examined (while only two taxonomic groups were involved), and combining pest consumers at taxonomic levels too course for isotopic analyses. Habitat variables' roles and how they were characterised are also not adequately described while samples (farm numbers) were not consistently balanced to justify temporal-scale comparisons. In addition, results of MixSIAR modelling that form the basis of the discussion and conclusions drawn are presented in form of means and standard errors while the conventional practice is to present medians and credible intervals. There are some inconsistencies (or gaps) in some non-MixSIAR analyses of data, particularly with regression, and a few non-clarifications on the study's objectives.  
Most of my comments are provided in the attached PDF of the article itself, but below are a general outline of these, by section.  
  
General comments/suggested changes  
Title:  
The current title implies that all generalist predators were studied, which is not the case - only spiders and lady beetles were considered  
Suggested change: Spiders and lady-beetles consume higher proportions of rice pests at late growth stages regardless of farming system. 1) We would like to keep GAP because… 2) It is common (backup by other paper examples)  
This is because there were only 2 predator groups examined here: spiders and lady beetles  
  
Abstract  
1. This is generally well summarised, but elements of it and some wordings/sentences will change after some of the results-presentation suggestions are addressed.  
2. Also the claim of predators being specialist towards crop maturity is unsupported by the results Please see our earlier reply.  
  
Introduction  
I have made specific comments and suggestions on the PDF attachment  
But in addition,  
1. The objectives stated but are entangles with the overall study goal; these need to be disentangled  
2. The objectives imply work on generalist arthropods whereas only 2 groups of these (spiders and ladybeetles) were examined; mots of the other generalist predators in rice-field systems (ants, ground beetles, earwigs, crickets, predatory bugs) were not part of the study  
3. No hypothesis is stated, and while this might not be absolutely necessary, it would be useful to add a small bit about what the study's expectations generally were, for the conclusions to be viewed by the general readership from the perspective of whether or not such expectations were met/achieved  
4. The role of 'years', in my opinion would not make a significant value since the rice agronomic practices do not change much from year to year n the study area (Taiwan). Therefore the crop stages are enough as a time-based parameter. 1) One of our big findings/contributions: Climate varied a lot over year, but GAP played a similar role (consistently high consumption of rice pests)   
5. There is need to clearly distinguish amongst: overall goal/aim(s); specific objectives; the study's expectations or working hypotheses. As at now, they seem to be all mixed up towards the end of the introduction section  
  
Methods  
1. Three farms in year one and 7 farms each in year 2 and 3 amounts to unbalanced sample size  
2. The authors need to more fully and clearly describe how mist-netting as conducted: it's not enough to say this was conducted while walking along ridges, because this implies sampling only insects along he field edges, rendering the collected samples un-representative  
3. Sweeping for canopy insets also implies that insects on other aerial parts of rice were ignored: not all pests are to be found on rice canopies (under-representation or under-sampling) We agree that each sampling method comes with limitation; however, we captured the most important / dominant rice arthropods.  
4. Also how plots were selected for sampling including distance separation between sampled plots, and how this was independent of arthropod movement patterns (to eliminate pseud-replication) will be desirable  
5. It would have been useful if the study assessed the role of some surrounding vegetation on field margins (eg on ridges and levees) as a food source alternative to rice, and how it compares to rice as a contributor to pest diets. This is because It could be that the reason predators consume more rice prey in late stages is that at this time, drier conditions and no water support little growth of alternative plants such as on ridges and levees, making herbivores move from there and be more abundant in rice. Consequently, predator-prey interactions are enhanced on rice, in which case the key explanatory factor here is therefore the watering regime rather than the crop stage. In the absence of this data/results, the authors need to adequately describe the structure of this kind of vegetation, including its persistence across the grow-out period and water availability, for there to be a clear picture as to why it might not have affected predator or pest dispersal or consumption rate results presented  
6. The role of habitat variables in driving predation rates has been treated very subliminally: not included in the objectives, not thoroughly described in Methods as to how the habitat structure (forest cover, other vegetation on field margins etc) were measured.  
Mention is made of forest cover, but no details as to how this was measured either, other that GoogleEarth was used for estimates. How close within the 1-km buffer were the forests to the rice-fields, for there to be an expectation that arthropods they might influence predation rates on farms? A 1-km buffer around farms is quite wide and though a few mobile arthropods may disperse from there into farms (certainly not spiders and lady-beetles!!!), it will definitely not affect pest-predator trophic interactions  
  
Analyses  
1. Combining/pooling pest consumers at order level seems very course for isotopic signature resolution. In stable isotopic analyses, pooling up consumers' food sources into similar guilds is usually a generally acceptable and realistic strategy which helps in saving time, effort and costs without risk of losing signature resolution for food source signatures in predators, for instance. However, this should usually be done at as low a taxonomic level as possible, say genus or family. Pooling food sources at order level seems like a rather wide stretch, not least because at that level, despite guild-commonality, feeding systems are so variant as to erode isotopic signature resolution roles of component groups in contributing to consumer diets. In your case, grouping grasshoppers with beetles is rather strange, especially as you said earlier that former has no trophic link to rice. Putting hemiptera and lepidoptera is equally awkward since some hemiptera are generalist predators or onmivores. Finaly, omitting crickets as part of detrivores is curious  
2. Mention is made of beta regression but not probability distribution or link function applied, or whther this was conducted using GLM or GLMM modelling  
3. ANOVA procedure is stated to have been applied to analyse some data that were supposedly analysed using beta regression. Why would such repetition be necessary?  
4. From the unbalanced sample size 3, 7 and 7 farms) across the three years, assessment of the role of years would have requires a statement as to how such imbalance was addressed before analyses were performed in beta regression, eg incorporating a Kenward-Roger approximation with an autoregressive data structure, so as to reduce the impact of data heterokedascicity  
5. It is also not clear whether the interactive influences of the various explanatory factors were performed on mean, medial or other values of food-source proportions from MixSIAR, of from other datasets. This should be clarified  
  
Results  
1. The results of the isotopic mixing models for pest contributions to predator diels should be presented in form of Medians accompanied corresponding by credible intervals, instead of Means and standard errors as the authors have done. This is because normally, the mean is more relevant when aspiring for a definite hypothesis test with normally-distributed (or transformed data) to reduce skewing and so we can test to get a p=value 0.05 etc) because mean is very sensitive or vulnerable to errors arising from long statistical-distribution tails. By contrast, with the Bayesian MixSIAR modelling, food proportion estimates, are almost always skewed so a measure of means is even more vulnerable to such impacts of tails (skewing), and the better option is therefore to use the median, which has the least sensitivity to skewed tails (compared to means or modes). But one has to provide the credible intervals to accompany the medians, just as you need SD (not just SE as the authors have done here) to accompany means in direct hypothesis tests. Therefore, the authors need to go back to their MixSIAR model results isotopic results and construct results based on medians. For details on this, see various articles by Brian Stock and Semmens.  
2. The results of the medians should consequently be presented in at least 1 table, and also the figures should preferably be presented in form of posterior median density plots (usually generated automatically by MixSIAR) rather than the linear graphs presented here by the authors  
3. After that results change, the patterns and trends in food source contributions to consumer diets will change a lot, the authors will the need to re-write results section  
4. There is not justifiable case for implying that because proportions of pests in generalist predators diets are relatively higher towards crop maturity, these predators then become specialists. You would have to quantify each of the component herbivores individually to see if they are consuming only one type at that stage (diet specialization/monophagy)  
5. The role of habitat variables in driving predation rates have not been clearly treated in results, so it is not easy to see how they influenced observed predation patterns. They could well be eliminated from the paper  
  
  
Discussion  
6. I have largely hesitated to review this section due to the changes that will likely results from the presentation of results of Median instead of Mean proportional contributions of pests to predator diets because after that results change, the patterns and trends in food source contributions to consumer diets will change a lot, the authors will the need to re-write discussion and conclusion sections  
  
  
#AU\_AGEE#  
  
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*In compliance with data protection regulations, you may request that we remove your personal registration details at any time.* [*(Remove my information/details)*](https://www.editorialmanager.com/agee/login.asp?a=r)*. Please contact the publication office if you have any questions.*